

I've received two sets of review comments on your submission "The Marginal Damage Costs of Different Greenhouse Gases: An Application of FUND." Both reviewers agree that this paper has the potential to make an interesting and significant contribution to the literature, but both also have serious misgivings about the paper in its current form. I would therefore like to encourage you to revise the paper to address the reviewers' critiques and resubmit the paper.

I would like to highlight a few of the most important concerns raised by the reviewers.

Reviewer #1 is concerned that the atmospheric chemistry in the model is overly simplified and out of date. The reviewer notes that methane and N₂O lifetimes vary with ocean chemistry (a dependence included in simple climate models such as MAGICC). (I would note the importance also of the influence of methane on aerosols; e.g., Shindell et al., 2009, 10.1126/science.1174760.) The reviewer is also concerned that the carbon fertilization included in the model is based upon literature that is 15–20 years old.

I personally would also suggest that greater attention be paid to the role of ocean acidification—which, while challenging to monetize, acts (perhaps quite strongly) opposite to the direction of carbon fertilization effect discussed extensively in the paper and is currently mentioned only as an aside in the last sentence.

Reviewer #2 would like the paper to be placed in the context of the broader literature on metrics. I would suggest that you consider calculating other metrics alongside GDP using the same assumptions as in your GDP calculation; this would make it possible to assess, for example, where your assumptions about methane radiative forcing place you relative to the assumptions used more conventionally to calculate GWPs. This would go part—but not all—the way to addressing the concerns about transparency raised by reviewer #2.

I am less concerned by reviewer #2's comments about the relevance of GDP; although European and UNFCCC climate policies are currently dominated by cost-effectiveness, cost-benefit analysis has an important role to play in U.S. Federal regulatory policy and elsewhere. (Thus the motivation for a special issue on the social cost of carbon.)

Following reviewer #2, I would suggest that you focus your methods descriptions in this paper on aspects important to the multi-gas comparison—e.g., how you are modeling atmospheric chemistry—and avoid repeating more general aspects described in other papers in the special issue.

In short, the reviews do indicate that more work is needed on this paper before publication, with a particular eye toward assuring that the modeling and discussion is better placed in the context of the current (atmospheric chemistry and metric) literature and that the relevant aspects of the model are described in a more transparent fashion. If you can revise the paper with these goals in mind, while also providing a detailed response to the reviewers' critiques, I believe the paper will serve as a solid contribution to the special issue and to the broader policy discussion.